Reply: Uncertainty in LCIA of Toxic Releases

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Dear Editor,

I appreciate the comment of DAVID PENNINGTON on my article "Uncertainty in Life-Cycle Impact Assessment of Toxic Releases" (Vol. 3, No. 5, pp. 246 – 258, 1998), since it gives me the opportunity to clarify some points that may have lead to misunderstandings. Following the sequence of his comments, I have the following remarks.

1) It is true that tools like EUSES reflect lengthy and thorough research efforts (see e.g. BORDING and SCHWARTZ (1998) for a review of the complexity of the model relations in EUSES). Yet, models are inevitably a simplification of nature. This problem is particularly relevant for the theme of toxicity, since the related complexities (number of substances, number of pathways, length of pathways, amount of data required, etc.) are considerably greater than those related to most other environmental themes. Risk assessment (RA) tries to correct for this by applying a tiered procedure. To avoid insurmountable efforts, simplified but conservative evaluation approaches are initially applied. If a risk is predicted, a more sophisticated analysis follows, and so on. To my knowledge, in this process tools like EUSES are used at best up to intermediate tiers (see e.g. fig. 8.16 in VERMEIRE and V.D. ZANDT, 1995). So in RA, EUSES may only give the final judgement if in the intermediate tier under conservative assumptions no risk is predicted. But otherwise further investigations are done leading to assessments in which monitoring data from the real world are preferred as an evaluation basis[1]. Model calculations tend to play a modest role (e.g. CALOW, 1998) – at least, I've not yet seen that calculated data were allowed to overrule field data[2]. Thus, in RA the application of EUSES is part of a procedure that aims to correct the differences between models and the real world. In LCIA, EUSES is the tool that gives the final judgement, and similar correction procedures cannot be applied.

2) Of course, EUSES takes the persistence of a substance into account when a predicted environmental concentration is calculated. But I meant that persistence is not seen as an evaluation criterion *in itself*, though it corresponds directly with the irreversibility of contamination. This is one of the key points in the controversy between industry and environmentalists on toxic substances. When contamination is reversible, corrections for new insights about risks can be made rather quickly by emission reductions. With an irreversible contamination this does not work. For instance, despite a full ban on CFCs the effect of ozone depletion will still be considerable for decades to come since the persistent CFCs released in the past are still present in the atmosphere. Precautious people thus will prefer reversible over irreversible contamination, also when LCIA-effect scores are equal. Indeed, this is one of the hidden value choices in current methods for LCIA. I refer to chapter 8 and 9 of my thesis for further examples (TUKKER, 1999). Such paradigmatic uncertainty, as I call it, should be sharply separated from the modelling problems mentioned by DAVID PENNINGTON. I wonder if his suggestion to classify substances prior to characterisation, reflecting regulatory approaches, is the best solution. Coverage by a regulatory approach is not necessarily the best criterion to trace substances for which the value choice is relevant. Moreover, the weight given to the irreversibility problem will be different for actor coalitions in society. Most probably, we will end up with weights that depend on the residence time of a substance, specific for each main actor coalition in society. From this point, it is only one step to the paradigm- or frame-related indicator system, I proposed in section 6 of the paper.

3) DAVID PENNINGTON's third point deals with (a) the possible intentional use of deterministic values in my paper; (b) the suggestion to use a probabilistic approach instead; and (c) the expectation that a probabilistic approach is feasible. I will deal with these elements in this order.

(a) As for the USES-input values we finally used, one has to remind that we performed the work in 1996, as a case-study, not a research project. To my knowledge, almost all LCA-manuals still give deterministic equivalency factors. Practical probabilistic approaches to LCIA have been proposed only recently during conference presentations (SETAC, 1998). Our study was probably the first that gave some kind of sensitivity analysis of the USES-based equivalency factors, and may still be one of the first practical cases in the peer reviewed literature[3]. So, it seems not unreasonable to say that the analysis went rather far for the time. Furthermore, one has to read the paper as a result of a historical process. First, we noticed that the results obtained with the LCA-USES method, released weeks before, were incompatible with rudimentary logic. Second, we decided to correct the flaws we then discovered by making an inventory of average literature values for relevant input parameters of USES. And third, an affected interest group dropped by, who found a weak spot in the USES-calculation and proposed an alternative approach (here for calculating BCFs) – not surprisingly leading to results more in their favour. This is quite a logical sequence of events occurring

[1] For new substances EUSES will always give a final judgement, since for these there is no alternative.

[2] I further refer to the recent warning of MITCHELL J. SMALL (1998) that models cannot be considered equal, let alone superior, to field data. He gave a nice anecdote. Two researchers noted that their models for stream re-aeration agreed with the observed field data with a correlation of about 0.7, but agreed with each other with a correlation of 0.8. "You see", they jokingly concluded "we agree on the truth, it is just nature that's a little out of step".

[3] Where, outside my and the journal's control, the review process took quite some more time than usual – without leading to a need for fundamental revisions of the paper's text.

without much intention from the practitioner's side. Indeed, it was no fun at all to explain top-level officials of the sponsoring company why such surprises occurred – particularly since the TNO/CML team I lead is probably one of the more experienced in the field of LCA, SFA and PVC. So, I feel it is rather a story where practitioners were taken by surprise by a lack of articulation of weaknesses in their method, than the reverse.

(b) Probabilistic approaches are, indeed, more sophisticated than what we did. However, I wonder if they provide the answer to all problems we faced. It is certainly not true, as DAVID PENNINGTON suggests, that our three sets of equivalency factors are part of a realistic probabilistic range. As indicated in the paper, the LCA-USES manual contains some five types of problems in input data (e.g. unrealistic defaults, like 160 days for the photo-degradation of VCM where literature gives 1-2 days). These are plain mistakes that have little to do with a normal variability around a realistic mean[4]. Furthermore, also the difference between the TNO/CML-set and the ECPI-set is not a probabilistic one. ECPI simply found a point in the whole sequence of calculations where they could suggest a systematic different way of calculating the equivalency factor (i.e. an alternative for calculating the BCFs of phthalates). Thus, Monte Carlo approaches will result in two distribution curves around the 'TNO/CML-mean' and the 'ECPI-mean'. Policy sciences shows that this 'experimenters regress' is a common phenomenon when vested interests are threatened. An interest group will simply look for a spot where the foundation of a calculation can be challenged in order to achieve a systematically different result. In LCIA of toxic releases, the potential spots where this can be done are numerous: the choice of classifying a chemical as carcinogenic or non-carcinogenic, the preferred way of extrapolating from high-dose levels in animal tests to low dose-levels relevant for standard setting, etc. I really wonder if variations in such systematic choices will result overall into a gaussian-alike distribution around a mean, and if it will be possible to predict in advance all the weak spots. Policy scientists even have come to the conclusion that uncertainty is *caused* by disagreement (since then stakeholders are motivated to look for weak spots), rather than that disagreement is possible if uncertainty exists.... (cf.. COLLINGRIDGE and REEVE, 1986).

(c) I agree with DAVID PENNINGTON that sensitivity analysis is useful to identify the most important parameters. However, I don't believe that uncertainty in LCIA of toxic releases can be reduced at trivial costs. Both in the Swedish PVC-chain study and the Dutch chlorine chain study we obtained a fairly reliable emission inventory for some 1,000 to 2,000 US $ per substance, despite the complexity of the systems and the number of processes involved. But in the Swedish PVC-chain study we needed 50% additional budget just to deal with the equivalency factors of 7 substances, and the problem was not yet solved. Obtaining just the OECD Screening Information Data Set (SIDS) for a substance costs 100,000 US $. If more sophisticated than SIDS-data is necessary this may even be some 4,000,000 US $ (US EPA, 1998). Obtaining these data for just the 3,000 High Production Volume Chemicals results in a price tag of 300 Mio to 12,000 Mio US $[5]. A programme in the lower price range most probably will not include field validation of models, which is highly relevant to solve e.g. the BCF-discussion we faced about phthalates. Therefore, I feel we talk here about efforts which are by no means trivial.

Messages like these may not be very popular. Yet, staying unaware of these realities is in my view the greater evil. Practitioners put their trust in LCIA-manuals and should be able to use lists with equivalency factors without too much thought. But in the current situation practitioners can end up in deep trouble, since it turns out to be uncertain and unknown if manuals indeed are as robust as they seem to be. The experience in the Swedish PVC-chain study and the Dutch Chlorine Chain study lead me to the conviction this is an unproductive situation[6]. If we feel indeed that the current uncertainty in fate modelling alone may be some 2 to 4 orders of magnitude[7], this simply should be stated in headlines in any manual. And if there is no really robust insight to what extent a model reflects the real world – insight that ultimately should be based on field validation – maybe it should not appear in LCIA-manuals at all[8]. The latter is in my view particularly true if a common sense analysis tells you that such yet not-quantified uncertainties can be enormous.

[4] Therefore, I feel that the only appropriate measure is an unambiguous withdrawal of the list of equivalency factors from the LCA-USES manual.

[5] The testing programme recently agreed upon between the US administration, the Environmental Defence Fund (EDF) and the Chemicals Manufacturers Association (CMA) will probably cost some 500-700 Mio US $ (Business and Environment, 1998).

[6] When TNO and CML performed the Dutch Chlorine Chain Study, the available LCIA method did not even include fate analysis. We paid rather little attention to this, since the manual's status was without question and commonly used. The severity of this blind spot became only clear when we had a meeting with risk assessment experts of industry, who shook their heads when they realised that in LCA toxicity generally was evaluated neglecting such a fundamental element as fate modelling, resulting in data with hardly any practical value. Also here we ended up doing additional research to produce something useful for policy making.

[7] At least, that was my personal impression of a conclusion at a workshop on Sophistication in LCIA held on 29 and 30 November 1998 in Brussels, in which some 60 LCA practitioners participated.

[8] I refer again to SMALL (1998) who stresses that unsupported 'Monte Carlo' simulations may be cheap and quick, but can never replace field validation.

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